records for 1909, 1915, and 1919, and other important matter for Galveston, Tex., have been furnished by the district engineer at Galveston, Tex.

Automatic tide records at Galveston for 1906, 1909, 1916, 1917, and 1919, and important references have been furnished by the Superintendent and other officials of the United States Coast and Geodetic Survey, Washington, D. C.

Reports from lighthouses in the Gulf, and the movement of buoys in storms have been furnished by the Inspector of the Light House District, New Orleans, La.

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THE FORECASTING OF SWELLS ON THE COAST OF MOROCCO.

By LOUIS GAIN.

[Abstracted from Revue général des Sciences, July 15, 1919, pp. 408-411.]

The great damage which was frequently wrought to shipping along the coast of Morocco by great ocean swells has been the subject of a number of studies. The author's studies have led him to the conclusion that these destructive swells can be forecast from the pressure distribution in the portion of the Atlantic to the east and northeast of Morocco. The conclusions, based upon the study of the effects of 210 low-pressure areas, are as follows:

I. A swell produced at Casablanca is the consequence of—

1. A depression on the ocean between the Azores and the British Isles, and light northwest winds in the region between the African coast and the depression. If the depression is intense, the swells will be correspondingly greater. These waves originating within the LOW require from 2 to 5 days to reach the coast of Morocco.

2. A depression moving eastward between the Azores and Portugal. In this case the swells are rarely large at Casablanca. They require from 24 to 48 hours to reach the coast.

3. Secondary depressions arising from LOWS in the north, moving southward over western Europe from the region of Norway and the British Isles, and giving rise to depressions over the Mediterranean.

II. A swell is weakened or made ineffective at Casablanca—

1. When there is an anticyclone over the region between the coast of Morocco and the depression.

2. When the depressions pass north of the British Isles.

3. In the case where depressions descend upon Europe when passing between Norway and the British Isles.

4. When an intense LOW with strong winds moves rapidly eastward.

The forecasting of swells can be either made directly at Casablanca by means of comparison of the daily wireless reports from Paris with those of the preceding day, or at Paris; the forecast itself can be forwarded to Casablanca. The author considers that more study should be given the problem, but that it is now possible to avoid such catastrophes as have been experienced along the coast of Morocco.—*C. L. M.*

MEAN SEA LEVEL.

By D'A. W. THOMPSON.

[Abstract reprinted from *Science Abstracts*, Nov. 29, 1919, p. 504. Article in *Nature*, Aug. 21, 1919, pp. 493-495.]

The level of the sea, or more generally, the form of its surface, is the resultant of two kinds of forces after eliminating the effects of the tides. There is the action of the sea currents and densities (intrinsic forces); and that of wind and barometric pressure (extrinsic forces). Witting thus summarizes the effects of the extrinsic forces: (1) Every barometric distribution of any permanency produces a deformation of the surface of the sea. (2) The ascending slope so produced is not identical in direction with the barometric gradient, but deviates to the right in the Northern Hemisphere. (3) The amount of slope is greater than that which would correspond with the hydrostatic pressure, induced by the barometric distribution. With regard to the intrinsic forces we know enough to choose a point at sea where no movements are caused by the distribution of densities. This is the zero pressure level. A geodetic surface drawn through this point may be considered the datum level. Proceeding outward from such a point, Witting has calculated the hydrodynamical gradient due to densities, and added to it the effect of barometric pressures. He has found that levels thus calculated for the Baltic area agree to a surprising closeness with the determinations of precise levels.

The question of secular changes of level is beset with difficulties. But assuming the coast from Wismar to Pillau has kept at constant level, Witting mapped the changes in level in the Baltic from 1898 to 1912. Some minor fluctuations are related to seismic phenomena; e. g., there was an interruption in the general upheaval at the time of the Scandinavian earthquake, 1904. For some centuries past the elevation of the Fennoscandian lands has gone on at the present rate. The phenomenon has been approximately the same for 6,000 years, but during the Bronze Age and just after, it was possibly slower. The more ancient phenomena are difficult to discuss, as a damming up of the Baltic outlet would produce results similar to actual land elevation.—*W. A. R(ichardson)*.

THE WAVE-RAISING POWER OF NORTHWEST AND SOUTH WINDS COMPARED.[1]

I recall that sailors on the Great Lakes have claimed that a wind of a given velocity in winter caused a higher sea than a wind of the same velocity in summer. They attributed this to the fact that in summer the relatively cold water of the Lakes reduced the temperature of the surface air layers, producing a temperature inversion. As a result, a wind movement in the upper air layers, which might be strong at the height of the masthead, would be light at the surface of the water. In winter, on the contrary, the air is generally colder than the water of the Lakes, the air movement is felt down to the surface and causes high seas.

Perhaps a similar explanation may apply to the difference in wave-raising power of northwest and south winds, since in the northern hemisphere the former are apt to be the colder.—*H. H. Kimball.*

[1] Cf. February, 1920, issue MONTHLY WEATHER REVIEW, pp. 100-101.

DISCREPANCIES BETWEEN ÅNGSTRÖM AND SMITHSONIAN INSTRUMENTS.

By C. G. ABBOT, Director, Astrophysical Observatory.

[Smithsonian Institution, Washington, May 3, 1920.]

In the issue of the MONTHLY WEATHER REVIEW for November, 1919, Dr. A. K. Ångström has three papers of great interest. In one paper he gives comparisons which must be highly gratifying to all those who are interested in the constancy of the scale of radiation measurements. He shows that in the seven years, 1912 to 1919, there had occurred no appreciable change in the Ångström and Smithsonian scales relatively to each other. During this interval Smithsonian observers have made several unpublished comparisons against the Standard water-flow pyrheliometer No. 3, which also supported the constancy of the Smithsonian scale with very satisfactory accuracy of experimentation. Thus we may be sure, it seems to me, that no change in the scales on which pyrheliometric and spectrobolometric measurements have been made for many years has occurred so large as 1 per cent.

Dr. Ångström finds the Smithsonian scale to be 3.2 per cent above the Ångström scale. Of this discrepancy, he admits that 1.8 per cent is due to the two small sources of error which he discussed in a former publication. The other 1.5 per cent he is inclined to throw upon the Smithsonian scale.

In regard to this latter suggestion, I am only able to say as was said in Volume III of the Annals: "The system which we call 'Smithsonian Revised Pyrheliometry of 1913' rests on 72 comparisons on 20 different days of 3 different years with 3 standard pyrheliometers of different dimensions and 2 widely different principles of measurement, all capable of recovering and measuring within 1 per cent test quantities of heat, and all closely approximating to the 'absolutely black body.' The 72 comparisons, 40 at Washington, 32 at Mount Wilson, were made in 6 groups. The maximum divergence of the mean results of these groups is 1 per cent. Hence it is believed that the mean result of all the comparisons made under such diverse circumstances must be within 0.5 per cent of the truth. The probable error is 0.1 per cent. It is believed that this standard scale is reproducible by the secondary pyrheliometers with the adopted constants given to within 0.5 per cent."

In Volume III of the Annals the determination of the constants of the Standard pyrheliometers Nos. 2, 3, and 4, and the comparisons which have been made with them, are given with great detail from pages 55 to 72, so that readers will be able to see for themselves at every step how far the claim just quoted is justified.

It appears to me that before we can be warranted in admitting Dr. Ångström's suggestion that the Smithsonian scale is 1.5 per cent in error because it exhibits that degree of divergence from the corrected Ångström scale, we ought to have equally full details of measurements and comparisons on which the Ångström scale and comparison between it and the Smithsonian scale rest.

Especially I would call attention to these points:

1. Since the electrical resistance of the Ångström strips in the standard instruments is measured by a potentiometer device between points of known distance apart it would be possible, by making the Wheatstone's bridge measurement of the actual resistance between the terminals of the Ångström strips, to determine the actual distance through which the heating of the strip occurred rather than to make an estimation with regard to that distance, as was done by Dr. Ångström in his experiments which led him to the correction of 1.3 per cent.[1] This is very important, for he will agree that the mathematical theory of the subject shows that if the difference in length between the sun-heated and electrically-heated portions of the strip should be above his estimate of it the magnitude of the correction would very rapidly grow.

2. Since the width of the strip is only 2 mm., accuracy to 0.5 per cent demands that the width should be known to within 0.01 mm. In view of the presence of the particles of platinum black and of soot required for blackening the strips, is it possible to define the edges of the strips to within this degree of accuracy? Dr. Knut Ångström,[2] the distinguished inventor of the instrument, states with regard to this point: "Since the coating with lampblack leaves the edges a trifle rough, an error of 0.01 mm. in measures of the width evidently can not be avoided, which in the width of the strips here used may make an error of 0.5 per cent in the final value."

3. Although the measurements of Kurlbaum indicate that the effect of introducing the heat at the front of the strip when heated by the sun, as against introducing it through the body of the strip when heated by the current, produces but a small amount of error, is it quite certain that the blackening Dr. Kurlbaum experimented with is so nearly similar to the blackening of the Ångström strips that this correction is as small for the Ångström pyrheliometer as for the Kurlbaum metal foil? Dr. Ångström's computations lead him to admit 0.5 per cent for this effect. But the magnitude of it must depend on the intimacy of contact between each individual strip and its blackening. Is this known to be uniform and that negligible opposition to the flow of heat occurs

[1] Astroph. Jour., vol. 40, p. 279. It is by no means certain that the ends of the strips electrically were at the edges of the pole pieces visually.

[2] Astroph. Jour., vol. 9, p. 336.